THURSDAY, APRIL 27, 1882

CHARLES DARWIN

Very few, even among those who have taken the keenest interest in the progress of the revolution in natural knowledge set afoot by the publication of the "Origin of Species"; and who have watched, not without astonishment, the rapid and complete change which has been effected both inside and outside the boundaries of the scientific world in the attitude of men's minds towards the doctrines which are expounded in that great work, can have been prepared for the extraordinary manifestation of affectionate regard for the man, and of profound reverence for the philosopher, which followed the announcement, on Thursday last, of the death of Mr. Darwin.

Not only in these islands, where so many have felt the fascination of personal contact with an intellect which had no superior, and with a character which was even nobler than the intellect; but, in all parts of the civilised world, it would seem that those whose business it is to feel the pulse of nations and to know what interests the masses of mankind, were well aware that thousands of their readers would think the world the poorer for Darwin's death, and would dwell with eager interest upon every incident of his history. In France, in Germany, in Austro-Hungary, in Italy, in the United States, writers of all shades of opinion, for once unanimous, have paid a willing tribute to the worth of our great countryman, ignored in life by the official representatives of the kingdom, but laid in death among his peers in Westminster Abbey by the will of the intelligence of the nation.

It is not for us to allude to the sacred sorrows of the bereaved home at Down; but it is no secret that, outside that domestic group, there are many to whom Mr. Darwin's death is a wholly irreparable loss. And this not merely because of his wonderfully genial, simple, and generous nature; his cheerful and animated conversation, and the infinite variety and accuracy of his information; but because the more one knew of him, the more he seemed the incorporated ideal of a man of science. Acute as were his reasoning powers, vast as was his knowledge, marvellous as was his tenacious industry, under physical difficulties which would have converted nine men out of ten into aimless invalids; it was not these qualities, great as they were, which impressed those who were admitted to his intimacy with involuntary veneration, but a certain intense and almost passionate honesty by which all his thoughts and actions were irradiated, as by a central fire.

It was this rarest and greatest of endowments which kept his vivid imagination and great speculative powers within due bounds; which compelled him to undertake the prodigious labours of original investigation and of reading, upon which his published works are based; which made him accept criticisms and suggestions from any body and every body, not only without impatience, but with expressions of gratitude sometimes almost comically in excess of their value; which led him to allow neither himself nor others to be deceived by phrases, and to spare neither time nor pains in order to obtain clear and distinct ideas upon every topic with which he occupied himself.

One could not converse with Darwin without being reminded of Socrates. There was the same desire to find some one wiser than himself; the same belief in the sovereignty of reason; the same ready humour; the same sympathetic interest in all the ways and works of men. But instead of turning away from the problems of nature as hopelessly insoluble, our modern philosopher devoted his whole life to attacking them in the spirit of Heraclitus and of Democritus, with results which are as the substance of which their speculations were anticipatory shadows.

The due appreciation or even enumeration of these results is neither practicable nor desirable at this moment. There is a time for all things—a time for glorying in our ever-extending conquests over the realm of nature, and a time for mourning over the heroes who have led us to victory.

None have fought better, and none have been more fortunate than Charles Darwin. He found a great truth, trodden under foot, reviled by bigots, and ridiculed by all the world; he lived long enough to see it, chiefly by his own efforts, irrefragably established in science, inseparably incorporated with the common thoughts of men, and only hated and feared by those who would revile, but dare not. What shall a man desire more than this? Once more the image of Socrates rises unbidden, and the noble peroration of the "Apology" rings in our ears as if it were Charles Darwin's farewell:—

"The hour of departure has arrived, and we go our ways—I to die and you to live. Which is the better, God only knows."

T. H. HUXLEY

PROF. WIESNER ON "THE POWER OF MOVEMENT IN PLANTS"[1]

Das Bewegungsvermögen der Pflanzen: eine kritische Studie über das gleichnamige Werk, von Charles Darwin, nebst neuen Untersuchungen. Von Julius Wiesner. 8vo, pp. 212. Three Woodcuts. (Wien: Hölder, 1881.)

Light as a Stimulus.—In "The Power of Movement" (p. 458) we observed the heliotropic curvature of a number of seedlings placed at 2, 4, 8, 12, 16, and 20 feet from a lateral light, and we came to the conclusion from inspection that the difference in heliotropic effect was not proportional to the intensity of light which the different sets of plants received. We think that this fact shows that light acts as a stimulus in causing heliotropism; if it acted in a simple physical manner the effects would bear some closer relation to the intensities of the causes.

Wiesner criticises this conclusion, and says (p. 78) that the fact is capable of a simple physical explanation. Wiesner has discovered that when plants are subjected to extremely bright light, they do not bend so much as when the light is slightly weaker (optimum for heliotropism); then as the light becomes weaker still, the heliotropic curvature again diminishes. This fact is supposed to be explicable by assuming the existence of certain negatively heliotropic elements. As far as I understand him, Wiesner does not allude to this question when he speaks of there being a simple explanation of the relationship between

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[1] Continued from p. 582.

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intensity of light and amount of curvature; and an explanation which requires so large an assumption cannot be considered a simple one. I shall therefore at present neglect the existence of the optimum, and consider the heliotropic curvatures produced by degrees of light all below the optimum. Here we see plainly enough that the intensity of the light is by no means proportional to the curvature.

Now Wiesner's explanation is that "the difference of illumination on the lighted and shaded sides, which causes the unequal growth leading to heliotropism is not proportional to the intensity of the light." This statement is in opposition to the laws of Optics. If J is the intensity of light per unit of surface, illuminating the light side, then the amount of light absorbed in passing through the plant is a certain proportion of J (call it A J), therefore the illumination on the darker side is $J - AJ = J(1 - A)$; and the difference between the illuminations on the two sides is A J, that is to say, the difference is proportional to the intensity of the light. [The proportion between the two intensities is $\frac{J}{J(1 - A)} = \frac{1}{1 - A}$, which is constant.]

Wiesner might connect the heliotropic facts with his observations on the amounts of longitudinal growth under different intensities of light (Monograph, Part ii. p. 12 and following pages); but this is only pushing the difficulty one step further back. The fact remains that the way in which growth is checked by light does not resemble the effect of light on photographic paper, but is more like the effect of light on sensitive animal tissues.[1]

Sachs and his pupil Müller Thurgau made known long ago the important phenomena of "Nachwirkung," or *after-effect*. If a plant is laterally illuminated for a certain time and then placed in the dark, the curvature will be continued in the same direction as though light was still acting on it. Or if, instead of being darkened, it is simply turned round, so that the direction of the light is reversed, the plant will go on curving in the direction in which it has begun to do so, in spite of the light now illuminating the opposite side. This phenomena we compared with the after effect of light on the retina. But Wiesner will not allow this comparison, chiefly because he finds an analogy between the after effect of light on plants and the photo-chemical induction of Bunsen and Roscoe (Wiesner's Monograph, i. p. 66). But we may just as well say that the effect of light on the *retina* is comparable to photo-chemical induction. We should then be able not only to say that light may in some respects act in a similar way on plants and animals, but (thanks to Wiesner) we should be able to form a theory as to why this is so.

In the "Power of Movement" we recorded our belief that plants are sensitive to the successive contrast of light and darkness, and this seems to us another point of resemblance between the sensitiveness of plants and animals. Thus, plants which have been exposed to the daylight did not become so quickly heliotropic as others, which had been kept in darkness. Again, consistently with this view, we attributed the striking effects of intermittent light to the increased sensitiveness of plants kept in the dark.

With respect to the effect of intermittent light, Wiesner states that the heliotropism produced by intermittent light is not *greater* than that due to constant illumination. The fact that so much heliotropic curvature is produced, is supposed to be due to the after-effect of the periods of illumination being continued through the periods of darkness, and thus producing an effect equivalent to continuous illumination. This naturally occurred to us, and it may possibly be the correct explanation.

Our statement that plants are more sensitive to light if they have been kept in the dark, Wiesner considers to be well known to be true, in the case at least of etiolated plants. He explains it by the increased capability for growth which is produced by darkness. On the other hand, he quotes an experiment from his "Monograph" to show that a plant subjected to equal illumination on all sides is rendered more sensitive heliotropically than a plant grown in the dark. This fact he considers as quite destructive of our view on the effect of contrast. He explains the result as follows.

The turgescence of the cells of an organ offer a certain resistance to its curvature, and therefore anything that slightly diminishes the turgescence increases the power of bending heliotropically; and as bright illumination diminishes turgescence, it may be expected to increase the heliotropic sensitiveness. This is such a contradiction to his explanation of our results and to other statements of his, that I should be inclined to think that I have misunderstood him. But we have on the same page (p. 83) our observations explained as the result of the increased "capacity for growth" caused by darkness, while his own observations are explained by diminished turgescence, which must imply diminished capability for growth as its chief result. If Wiesner explains his own observations correctly, he cannot explain ours, and therefore his criticism is deprived of all force.[1]

Geotropism—Sensitiveness of Root-tips to Gravitation.—If the extreme tip of a root (1 to 1·5 mm.) be cut off, the root will continue to grow vigorously, but it ceases, or almost ceases, to be geotropic.[2] We concluded that when a root is placed horizontally, the force of gravity acts on the tip, and a stimulus is transmitted to the growing part of the root, causing it to bend down. And that accordingly when the root-tip is destroyed the root is incapable of receiving the stimulus of gravitation. Wiesner differs entirely from this point of view, and believes that the diminished geotropism of the pointless roots is simply due to their diminished activity of growth due to the injury. I have elsewhere treated this question experimentally, and have been able to show that if the tip is not cut off roots may be very severely injured in other ways, so that the growth is greatly retarded, and that they are nevertheless capable of geotropic curvature. I have thus shown that Wiesner's explanation of the facts cannot be the right one, since mere retardation of growth is not sufficient to account for loss of geotropism.

Sensitiveness of Root-tips.—In "The Power of Movements" we showed that the tips of roots are stimulated in a peculiar manner by the continued contact of small squares of cardboard cemented to one side of the extreme tip of the root. It was found that the root curved away from the side on which the card was fastened, so that in some cases the root made complete loops by its continued curved growth. We believe that the tips of roots are sensitive to contact, and that when a root meets with an obstacle in its way the pressure on one side of the tip causes the growing part of the root to grow more rapidly on the side of the obstacle, and thus curve away from it. It is conceivable that a root should curve away from an obstacle, owing to the forcible bending of the root, just as any other ductile object would give way when forced against a fixed object. It was from observing roots bending away from fixed obstacles placed in the way of their growth that we were led to suspect that the curvature was not due to forcible bending, but to a special kind of curvature, due to stimulus transmitted from the sensitive tip.

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[1] Of course no one denies that stimulation ultimately depends on the physical properties of the sensitive tissue.

[1] It should also be observed that Wiesner's experiments were made with etiolated plants, which was not the case with ours. It appears (Monograph, p. 8) that in *V. Faba* and *Soja hispida* no increase of heliotropic sensibility was produced by illumination.

[2] "Power of Movement," Chap. x. The original experiment on this point is due to Ciesielski.

Wiesner confirms our facts, but differs entirely in his interpretation of them. His explanation rests on certain facts observed by us, namely, that if the extreme apex of a root be wounded on one side by an oblique cut with a razor, or by a touch with lunar caustic, the same curvature occurs as when pieces of card are attached. Wiesner believes that the spirits of wine contained in the shellac varnish which was used in many of the experiments to fasten on the cards, injures the root on one side, and that this accounts for the curvature which we ascribe to contact. But Wiesner's criticism is incomplete on this point. He takes no notice of the experiments in which we used thick gum, instead of shellac varnish, for attaching the pieces of card to the tips of the roots. It is difficult to believe that gum injures the roots, since gum by itself produces no effect; and it seemed pretty clear that it was the card, and not the gum, which acted, for when, as often occurred, the gum absorbed water and swelled up so that the card was separated from the root, no effect was produced. Nor does Wiesner mention our experiment in which the shellac varnish was not attached directly, but to a small piece of gold-beater's skin applied to the root.

If Wiesner's view is correct, then since the thick shellac varnish which we used hardens on the surface in a few minutes, it is clear that the injury to the root must take place in this short period. This however has not been shown to be the case.

Wiesner makes a great point of an experiment in which he causes small pieces of wood (p. 144) and sand to adhere, by pressing them against one side of the root-tip without using shellac: under these circumstances he says that no curvature took place. This experiment does not seem to me so important as Wiesner would have us believe, but for reasons given in the next paragraph I defer further discussion of the whole question.

A few words must be given to some of Wiesner's other experiments on this subject. He caused roots to grow against various more or less yielding objects in such a way that he could estimate the pressure against the root-tip, and found that the root did not bend from the touching obstacle till the pressure is sufficient to cause forcible bending. Here Wiesner's experiments seem not to be quite conclusive, and I have begun to make experiments which I hope will prove to be crucial. But I have been obliged to stop, because of the curious want of sensitiveness in the tip of the root which has occurred in a large proportion of the beans tried. I have repeated the experiment of fastening on pieces of sand-paper with shellac to one side of the root-tip, and in the large majority of cases no curvature ensued. I have no means of knowing what the meaning of this failure may be (I am inclined to believe it is owing to the experiments being made in the winter), but it is clearly useless to pursue the experiments with such abnormal material.

Wiesner seems to be in doubt whether or not the curvature of the root away from the injured side implies that a stimulus travels from the sensitive tip to the growing part of the root, whether, in fact, the injury to the root acts as a stimulus. The alternative proposed by Wiesner (p. 145), and which he thinks more probable, is that in consequence of injury, "the uninjured part of the root gets into a state which directly increases its capability of growth without any stimulus coming into action." Wiesner's conception of stimulus ("Reiz") is not quite easy to understand, thus, for instance his definition of "Reiz" ("Elemente der Anat. und Physiol. der Pflanzen," 1881, pp. 249, 250) is an "influence whose mechanical efficiency (Leisting) is out of proportion to the resulting mechanical effect. A stimulus in this sense does not directly cause the movement, it merely releases the efficient forces, just as the pressure of a finger on the trigger of a gun produces the explosion indeed, but stands in no relation to the force with which the ball is driven through the barrel." In accordance with this definition he describes ("Anatomie," p. 253) the nocturnal (nyctitropic) movements of the leaves of Robinia as "a phenomenon of stimulation (Reiz-erscheinung) depending on differences of Turgor." Again, in his present work ("Bewegungsvermögen") he gives (p. 25) a definition of "Reiz," which is essentially the same as that already given. He adds, however, the characteristic of transmission to his definition:—"Every irritable part has the power of transmitting the stimulus to neighbouring parts which are also usually irritable." He goes on to say that the sleep-movements of leaves do not realise the characteristics of a Reiz-Erscheinung, thus contradicting what he says in his other work.[1] Whether we take the first or the second definition, it seems surprising that Wiesner should doubt that the curvature of a root when its tip is injured is a phenomenon of stimulation. If it were, as Wiesner is inclined to believe, the direct mechanical effect of the injury, the result would be curious; for if injuring one side of the root causes increased growth on the same side as the injury, it is clear that injuring both sides symmetrically ought to increase the whole growth of the organ. And accordingly when the tip is cut off by a section perpendicular to the axis of the root, its growth ought to be accelerated; which would be directly contrary to the results of Wiesner's experiments on this point.

Symmetrical injury to the tip looked at from our point of view could not of course be expected to lead to consequences of this sort.

The next chapter in Wiesner's book begins with the question of "spontaneous" nutations, and under this heading occur the hook-like curvatures of the ends of shoots of Ampelopsis. This curvature Wiesner considers to be due to the weight of the end of the shoot; we ascribed it to epinasty, because on horizontal and inclined branches the hooked tip does not necessarily point vertically downwards, but is often horizontal, or points upwards (Power of Movement, p. 272). Wiesner on the other hand states that all the ends of shoots observed by him at the season when the Ampelopsis is growing vigorously pointed downwards; this fact would gain in value if it were stated that the observations included horizontal branches. On the same page he remarks that the downward directed shoot often goes far beyond the vertical, and this, as well as the vertical position seen in other cases, is explained by "Zugwachsthum" (*i.e.* "growth produced by strain"), due to the weight of the shoot. How this can possibly be the case is not explained.

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[1] It seems to me inconceivable that a stimulus should act indirectly by releasing potential energy, as in the case of discharging a gun, unless some kind of transmission of the stimulus takes place; and if this is so, Wiesner's two definitions are identical.

Diaheliotropism.—The power possessed by many leaves of placing themselves at right angles to the direction of incident light was called *Transversal Heliotropismus* by A. B. Frank; we have called it diaheliotropism, partly for the sake of convenience and of uniformity in nomenclature, and partly because our view does not exactly coincide with Frank's. Wiesner asserts positively that diaheliotropism does not exist, and that all the phenomena can be explained as the result of the balance of ordinary forces, positive and negative, heliotropism, positive and negative, geotropism, epinasty, hyponasty, weight, &c.

The main features of Wiesner's explanation are as follows:—We are supposed to have a vertical bud whose leaves are bending epinastically down towards the horizontal position, which they ultimately assume when the light comes vertically from above. In this stage of growth before the position of the leaves is determined by that of the light, Wiesner speaks of the possibility of a balance being struck between epinasty and apogeotropism. But Frank (*Bot. Zeitung*, 1873, p. 22) has long ago shown the impossibility of a balance being struck between a constant force like epinasty, and a force (and this would apply to apogeotropism) which varies with the position of the organ with regard to the horizon.

To continue Wiesner's explanation: when the plant is exposed to a zenith illumination, the leaves bend downwards owing to apheliotropism, and if truly apheliotropic, would continue to bend till they pointed vertically downwards. But Wiesner believes that the light, besides causing apheliotropic movement in the leaves, has the power of checking their apogeotropism. If therefore the leaves in moving downwards go beyond the horizontal position, they become obliquely illuminated, and accordingly the light being weaker, the inhibition of apogeotropism is lessened, and the leaves rise up. What occurs if the leaves, in this upward movement, go beyond the horizontal is not explained, for in this case also the apogeotropism would be diminished. To explain this it would be necessary to make more assumptions as to the variations in apheliotropism due to the varying obliquity of light, and the variations in apogeotropism due to varying positions of the leaves with regard to the horizon; and no such assumptions are made by Wiesner. The faultiness of Wiesner's explanation is made clear by the following example. Let us assume that Wiesner's explanation holds good for zenith illumination, and suppose that a seedling dicotyledonous plant under these conditions is suddenly subjected to oblique light. Then both that cotyledon which is on the illuminated side, and the one on the shaded side will be obliquely illuminated, therefore, according to Wiesner, the apogeotropism of both cotyledons will be increased, and both should rise up; but what really happens is that one falls and the other rises. This seems inexplicable, unless we suppose that the apheliotropism of the leaves differs according as the light falls, as shown by arrow No. 1 or No. 2 in Fig. 2.

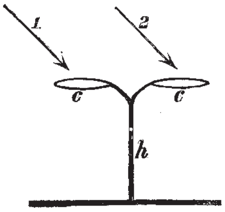

FIG 2.

FIG. 2.—Diagram representing a seedling dicotyledon exposed to oblique light. *h*, the hypocotyl, or stalk; *c*, *c*, the cotyledons; the arrows represent the direction of the light.

Wiesner's explanations are also shown to be untenable, at least in some cases, by my observations "On the Power possessed by Leaves of placing themselves at Right Angles to the Direction of Incident Light" (*Journal* of Linn. Soc., vol. xviii., 1881). I have there shown that certain plants are able to place their leaves at right angles to incident light when removed from the action of apogeotropism. These experiments are not discussed by Wiesner, and would seem to be inexplicable from his point of view. When engaged in this work on diaheliotropism I was struck with the impropriety of considering as heliotropic all movements towards a source of light, or all movements away from it as negatively heliotropic. Thus the leaves of *Ranunculus ficaria* move either towards or from the light (independently of gravitation), according as either of these movements is required to place the leaves at right angles to the direction of the incident light. And it is obviously impossible to call the leaves both negatively *and* positively heliotropic. It is far more rational to call them diaheliotropic, and there is no more objection to the use of this term than there is to the terms heliotropism or apheliotropism; all such terms are confessions of ignorance, and none of them exclude further research into the phenomena to which they are applied.

Diageotropism.—As a diaheliotropic organ is one which possesses the power of placing itself at right angles to the direction of incident light, so a diageotropic organ is one which possesses the power of growing at right angles to the line of gravitation. Thus certain underground stems possess the power of growing horizontally beneath the surface, instead of vertically upwards, like most stems.

In Wiesner's few remarks on this subject, he completely ignores Elfving's remarkable paper on horizontally-growing rhizomes, which we especially referred to, and which is by far the most striking evidence which we possess in favour of the existence of diageotropism.

Hydrotropism.—Roots have the power of bending towards a wet surface, and we have shown, that when the tip of the root is covered with a layer of grease, the root does not usually bend towards the wet surface, from which we inferred that the sensitiveness to moisture resides in the tip. Wiesner believes that the treatment to which the tip of the root is subjected, lessens the power of growth of the root, and that the loss of hydrotropism is a consequence of the less vigorous growth in the root; this is practically the same criticism as that which Wiesner has applied to our views on geotropism; namely, that the lessened rate of growth caused by the injury to the *punctum vegetationis* interferes with the power of geotropic curvature.

Wiesner has, however, himself observed the fact that the *less* turgescent roots are those which seem to exhibit hydrotropism best, and as want of turgescence would certainly interfere with normal growth, fully as much as the grease on the tips of the roots, Wiesner's criticism seems to be considerably weakened.

In a notice of the present extent, it would be impossible to notice all Wiesner's experiments and arguments, many of which possess much interest. To do so, would require a whole volume such as Wiesner has devoted to the subject, and to which I must refer those who wish to be better acquainted with his views. Finally, I would ask those who do so, not to forget to refer to "The Power of Movement in Plants," for it is only by studying the two books together, that an adequate opinion on the questions at issue can be formed. FRANCIS DARWIN

OUR BOOK SHELF

Catalogue of the Batrachia, Salientia and Ecaudata in the Collection of the British Museum. By George Albert Boulenger. (London: Printed by Order of the Trustees, 1882.)

THIS volume is a proof of the steady though rapid progress which our great Natural History Collection is making, and is a token as well that under the present keeper of the Zoological Department, the stores of specimens will be made fully available for scientific reference. To the working zoologist there can be no more acceptable gift than such carefully compiled catalogues, and by the publication of such the collection itself not only indirectly but even directly benefits, for an interest is awakened in the objects described, and thereby the stream of donations begins to flow. The first edition of this Catalogue, published in 1858, contained the description of some 283 species, and the collection consisted of some 1691 specimens. The present edition contains the description of 800 species, of which the British Museum possesses 522, represented by some 4692 specimens. The first edition, by Dr. Günther, without doubt gave a great impulse to the study of the tailless Batrachians, and as a result it is now out of date. For the present edition Dr. Günther has been fortunate in securing the services of Mr. Boulenger, the assistant naturalist in the Royal Museum of Belgium, and exceedingly well has the latter accomplished his task. The classification adopted is based on that of Mr. E. D. Cope, somewhat modified, and biologists in general will be pleased to know that this classification seems to harmonise not only with the natural affinities of the genera, but with all that is known of the geographical distribution, development, and physiology of the group. The only serious objection urged against it is the supposed difficulty of ascertaining on the recent specimens the osteological characters, without sacrificing one or more specimens; but, as Mr. Boulenger asserts, it requires only a moderate skill and two or three clean incisions to reveal all the required secrets of the structure of the vertebræ.

One important feature in this catalogue is that we find in it an account of all the known species of the group, so that it to a great extent possesses the merit of being a monograph, and it thus indicates the species which are wanting in the National collection. There is also a very considerable beginning made in the descriptions of the larval forms, and sometimes these are figured, the collecting of these forms we trust may receive a fresh stimulus from the publication of this work.

Mr. Boulenger well merits the confidence with which Dr. Günther writes that "zoologists will thankfully acknowledge the industry and ability with which the author has performed his difficult task."

It only remains to add that this catalogue is illustrated with numerous woodcut illustrations, and with thirty lithographic plates.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

On the Conservation of Solar Energy

WITH your permission I should like to offer a few remarks upon the interesting paper of Dr. C. William Siemens on the "Conservation of Solar Energy," published in NATURE (vol. xxv. p. 440). The main hypothesis upon which that paper is based, that of a fan-like action of the sun, is not improbable; nor are the consequences drawn illogical, if we could reasonably imagine space to be occupied by such condensed molecules as he supposes. That space is everywhere occupied by matter, there is no just reason to doubt. The hypothesis of an ether, specifically distinct from matter, is a gratuitous assumption, and one of the last surviving relics of eighteenth century science. Unless it can be proved that highly disintegrated matter is positively incapable of conveying light vibrations, there is no warrant for assigning this duty to a distinct form of substance. But that matter exists in outer space in the same conditions as in the planetary atmospheres is certainly improbable. Its duty as a conveyer of radiant vibrations seems to require a far greater tensity, and its disintegration is probably extreme.

If we assume, then, that matter exists throughout the universe, here as condensed spheres, there as highly rarefied substance, with the atmospheric envelopes of the spheres gradually shading off into the excessively rare matter of mid space; another hypothesis may be deduced, somewhat different to that offered by Dr. Siemens. The views which I desire to present have been already published, but they seem worthy of repetition in connection with his solar theory.

On the Nebular hypothesis, the matter of the sun was once disseminated through space. Gravitative attraction has, therefore, had a double effect. The greater portion of this matter is now drawn together into a contracted mass. The remaining portion yet occupies outer space, in a far more rarefied condition than the original. But an important consequence attends the condensation and rarefaction of gases. This is, that condensed gases become heated, rarefied gases cooled, and this without the aid of heat exchange with outer material. In the one case a portion of the absolute heat of the gas, formerly latent, becomes sensible; in the other a portion of the sensible heat becomes latent. If originally the absolute heat contents and the temperatures were alike equal, condensation and rarefaction would not, of themselves, change the heat contents, but they would change the temperatures. In condensation, the latent heat is reduced, the sensible heat increased, and the temperature rises. In rarefaction the opposite effect is produced, and the temperature falls.

This consideration applies as well to the problem of the condensation of nebulous as of terrestrial gases. The effect of contraction of nebulous gas into a dense sphere, must be a considerable rise in temperature if there be no diminution of absolute heat contents. The effect of rarefaction of the remaining matter of space must be a decrease in temperature. Thus if radiant outflow of heat from the sun had been prevented during its condensation, the eventual result must have been that the sun and the matter of outer space would have continued equal, mass for mass, in absolute heat contents, and yet have become immensely different in temperature.

And from this must have come another interesting result,